THE

AËRONAUTICAL JOURNAL.

(FOUNDED 1897 in succession to the ANNUAL REPORTS.)

THE ORGAN OF THE ROYAL AËRONAUTICAL SOCIETY.

Published Monthly at the Offices of the Society, at 7, Albemarle Street, Piccadilly, London, W.1.
Telephone: "Gerrard 7373." Telegraphic Address: "Didaskalos, Piccy, London."
Subscription per annum, £1 12s. 6d.; single numbers, 2s. 9d., post free.

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All communications should be addressed to the Editor.

No. 142. OCTOBER, 1922. VOL. XXVI.

Notices of the Royal Aëronautical Society.

New Chairman.

On the termination of the term of office as Chairman of Lieutenant-Colonel Mervyn O'Gorman, C.B., Professor Leonard Bairstow, C.B.E., F.R.S., becomes Chairman of Council on the first of this month for the year 1922-1923.

Election of Members.

The following members were elected at a Council Meeting held on Wednesday, September 13th :—

Associate Fellow.—Miss H. M. Lyon.

Student.—H. A. Sherwin Gothard.

Presentation of Awards.

The Society's Silver Medal will be presented to Mr. H. R. Ricardo, and the Pilcher Memorial Prize for Students to Mr. S. H. Evans at the commencement of the first meeting, to be held at the Royal United Service Institution on Thursday, October 5th, at 5.30 p.m.

Journal.

Among the advertisement pages will be found a classified list of the articles which have been published in the Journal during the last five years, which it is hoped will prove of value for reference purposes. A number of copies of this list have been printed off as a leaflet, and members will greatly assist if they would forward to the Secretary the names of anyone they think likely to subscribe to the Journal to whom this leaflet might be sent.

In connection with this, the following quotation from a letter recently received from America is of interest :—

> "I congratulate you and the Society on the continued excellence of the AERONAUTICAL JOURNAL, which is to-day quite the only aeronautical publication which is worth reading carefully."

Library.

The following books have been received and placed in the library :—"Fuel for Motor Transport," Second Memorandum, by the Fuel Research Board. "A Short Course in Elementary Meteorology," by W. H. Pick. "Proceedings of the Second Air Conference, held on 7th and 8th February," Air Ministry. "All

the World's Aircraft," edited by C. G. Grey. "Aeronautic Papers," by J. H. Parkin. "A Dictionary of Applied Physics," Vols. 1 (Mechanics, Engineering, Heat) and 2 (Electricity), edited by Sir Richard Glazebrook. Reports of the First International Air Congress (Paris, 1921). "Report on Ex-German Aerodromes and Material in Back and Occupied Areas," by the Inter-Allied Aeronautical Commission of Control. "Aviation in Peace and War," by Sir F. H. Sykes. "Application de la Resistance des Materiaux au Calcul des Avions," by M. Boileve. "The War in the Air," by Sir Walter Raleigh. "14,000 Miles Through the Air," by Sir Ross Smith. "Steel Thermal Treatment," by J. Urquhart. "H. G. Hawker, Airman," by M. Hawker. "Theory of Wave Transmission," by George Constantinesco.

Forthcoming Arrangements.

Oct. 5, 5.30 p.m.—Royal United Service Institution. Prof. L. Bairstow, "The Work of S. P. Langley." Presentation of Awards.

,, 11, *Scottish Branch.*—Annual Meeting. Mr. Sholto Sheppard, "Proposed Solution of Aerial Transport Problems." Mr. A. A. Sidney, "The New Beardmore Engine."

,, 12, 6.45 p.m.—Society's Library. Students' Section. Annual Meeting and Election of Officers.

7.30 p.m.—Inaugural Address by Dr. A. J. Sutton Pippard; Chairman, Lieutenant-Colonel W. Lockwood Marsh. (This meeting is open to all members of the Society.)

,, 17, 4.0 p.m.—Library and Publications Committee.

4.30 p.m.—Candidates' Committee.

5.0 p.m.—Council.

,, 19, 5.30 p.m.—Royal United Service Institution. Mr. J. D. North, "The Metal Construction of Aeroplanes."

,, 24, *Scottish Branch.*—Sir Sefton Brancker, "Air Transport To-Day and To-Morrow."

Nov. 2, 5.30 p.m.—Royal Society of Arts. Major A. R. Low, "A Review of Airscrew and Helicopter Theory, with Aeroplane Analogies."

,, 9, 7.30 p.m.—Society's Library. Students' Meeting. "Airships," by Mr. H. C. Brown. Chairman, Lieutenant-Colonel W. Lockwood Marsh.

W. LOCKWOOD MARSH, *Secretary.*

WILBUR WRIGHT LECTURE.

The tenth annual Wilbur Wright Memorial Lecture was delivered at the Royal Society of Arts, at 5.30 p.m., on Thursday, June 15th, 1922, by Lieut.-Colonel A. Ogilvie, C.B.E., Fellow.

SOME ASPECTS OF AERONAUTICAL RESEARCH.

It was the wish of the Council of the Royal Aeronautical Society that this year's Wilbur Wright Memorial Lecture should have been prepared and read by an American, in order that the practice of drawing the annual lectures alternatively from Great Britain and America might be more firmly established as a custom.

Unfortunately the arrangements made for this fell through, and you will therefore have to put up with one who, though fully conscious of the eminence of his predecessors, is proud that he has been granted this opportunity of showing his respect and affection for Wilbur Wright.

Wilbur Wright.

When ten years ago a telegram came from Orville saying that Wilbur had died I felt as if I had lost an elder brother, who was not only a past-master on the subject of flying, a master willing and anxious to impart anything he knew, but also one who would give careful, painstaking and entirely unselfish consideration to any matter, however unimportant. I never knew him to give a hasty or ill-considered judgment about anything or any person.

It was in December, 1908, that I first met him, when he was at Le Mans, demonstrating to the European public at large, and to the French in particular, that the doubts and suspicions with which the statements of themselves and their friends had been received were without foundation and that real flying was an actual accomplished fact.

I had gone to France in order to learn as much as possible about the design and construction of an aeroplane before setting out to build one of my own, and after visiting the Salon and the works of all the notable constructors, went out to Le Mans to see the Wright machine.

There were a thousand or more people waiting at the aerodrome on the chance of seeing something, and after an hour or two, before our eyes there actually happened what we had heard about but had only half believed. My diary reads as follows:—

> "W. Wright made two wonderful flights in afternoon, all kinds of evolutions, turning, skimming close to ground, gliding without engine from 200 feet. A true and complete flying machine. No chance of speaking to Wright."

Returning a few days afterwards, I arrived to find Wright circling the aerodrome for the Michelin Cup. After a time he was brought down by some small defect in the oiling system.

A mere trifle had prevented him from making a two-hour flight which would have marked for him an important stage, and there was only one more day to go before the end of the year. Nevertheless, while packing the machine away into its rough shed, deciding upon and carrying out a cure for the lubrication trouble, he was quite calm and unhurried.

After everything was in order for the next day we had a long talk, walking about the frozen aerodrome and discussing his plans and mine. One could not help being impressed by his absolute honesty, sincerity and self-control, as well as by his obvious intellectual powers, and I conceived a very great admiration for his character.

It was impossible to evade the thought that he was a man apart, and so I always thought him, notwithstanding the growth of our personal relations into a very warm friendship.

The genial warmth of this I should like to illustrate by a little incident which happened at Eastchurch in 1911, when he was stopping in camp with me and helping to get my little racing machine ready for the Gordon Bennett.

We were running the engine in the shed with the doors closed, and Wilbur was testing the propeller revolutions by counter and stop-watch. He was wearing a bowler hat, and when withdrawing his head and shoulders through the front spar bracing, managed to get the hat knocked off his head by a wire. It rolled across the plane and was caught by the propeller, flew round the shed, came back into the propeller stream and made a second circuit, finally coming to rest in a corner. After this trip the hat was considerably dented, so we bought him a new one in exchange.

The following reference from a letter shows the pleasant humour and kindly spirit which so endeared him to his friends :—

> " P.S.—When I came to settle accounts with Mr. Brewer I found several disputed accounts, in one of which he claimed you were the only person interested. I claim that if an old hat increases in value in proportion to the number of holes put into it that I have a right to see if there is any possible chance of getting in one or two more before trading it off ! "

The Foundations of Knowledge.

One of the main points which I want to bring out in this paper is that Wilbur and Orville Wright based the whole of their knowledge on solid foundations, and that if they had not done so it would not have been possible for them to have overcome the difficulties which they encountered before they demonstrated to Europe and to America that real flight was an accomplished fact. It is my firm belief that without that demonstration and inspiring example, the aeroplane as we know it to-day would not exist.

It is also my firm belief that our rapid technical development during the war period, in which we as a nation overtook both friends and enemies after starting a long way behind, was mainly due to the solid research work which was done in the laboratories of this country between the years 1909 and 1914.

It appears to me, however, that there is some danger that the real lessons of the past have not been understood and taken to heart, and I therefore take leave to conduct you again over some old ground that we may see if there is not something there which will help us in laying our plans for the future.

It has been generally accepted that the Wrights were experimenters who grasped the essentials of the problem of flying, as it was then known, and tackled them in a practical and intelligent way. Also that they were the real and true pioneers of mechanical flight in December, 1903.

But there are still men, eminent and distinguished men, who believe that this success was really due to the superior flying skill and technique of the two brothers. They do not appreciate that it was a demonstration of something bigger and more important even than that—a demonstration of a firm structure of knowledge based on experimental work of the most solid kind.

It is greatly to be deplored that it has not been possible to publish the results of the wind-channel work which was done in the workshop at Dayton after the 1901 gliding was over, because the actual figures of these experiments would have dispelled the last doubt.

However, a study of the results attained affords to an aeronautical engineer evidence which should be sufficient to convince him.

Take the small racing machine which in 1910 the Wrights produced to compete in the Gordon Bennett Cup and compare it with their standard machine of that date, and it is obvious, even more so now than it was then, that both machines were designed on a scientific basis founded on definite data. Table A gives the main figures for the two machines, so that they are easy to compare.

You will notice that the aspect ratio is about the same in both machines, *i.e.*, about 6 to 1, and the gap chord ratio about 1, both of which features are still standard practice to-day.

The bold increase in the surface loading from $2\frac{3}{4}$ to 6 lbs. per square foot should also be noted.

I was in Dayton during the time the small machine was being designed and built, and saw how reference was continually being made to little pocket-books, in which were collated the results of the wind-channel research work carried out mostly during the winter of 1901-1902.

It was known that the French flyers would be sending over a strong team to capture the cup and that they would be using the new 70-h.p. Gnome, an engine of good power for weight, and one which was considered in those days to be a very formidable proposition and by many almost too powerful for a single-seated machine. This may be amusing to my present audience, but I would remind them of the excitement when the 50 Gnome was put into the Bleriot monoplane in place of the 25 Anzani.

The Wrights made up their minds that they would have to double the speed of their standard machine in order to win. They were much behind time, and had to make the utmost use of their standard equipment. They therefore designed and built a machine of less than one-third the area, two-thirds of the weight and double the power of their standard machine.

When first flown by Orville the machine came up to its designed performance —was in perfect balance and control.

It is doubtful if any aeronautical designer of the present day, with all the increased knowledge now at his disposal, could make so immediate a success of so radical a departure.

It has been my fortune to have had a great deal of experience in the trials of new designs of aircraft and to have watched many through their teething troubles, but looking back on that performance of twelve years ago it is evident that a very full knowledge of design data was available to the Wrights at that date.

History of Experimental Work.

To get a grasp of the processes of research and technical development, through which Wilbur Wright and his brother passed, it would be as well to recall briefly the history of their experimental work, looking at the matter from this particular angle. In 1900, after absorbing all available information on the subject of flight and following particularly the path indicated by Lilienthal, they built their first glider with biplane wings 18ft. span by 5ft. chord, incorporating a warping device for lateral control and a front elevator for fore and aft control.

Some of you might be interested to hear how this warping device was thought of.

The Wrights knew that they wanted to give different angles of incidence to the two wing ends to obtain the lateral control which they saw from accounts of previous experiments was necessary. Wilbur was explaining to an interested audience what was wanted and was using an old open-ended cardboard box,

which had probably contained puffed wheat or some other strange American breakfast food, to illustrate his remarks. As he was holding it by the two ends and twisting it about, he suddenly realised that in his hands he had a biplane structure firmly braced in two planes but able to be twisted at the two open ends, and that this was just what was wanted for the lateral control of a flying machine. In the first machines the warping wires ran in the fore and aft direction and not forming the bracing of the back spars in a lateral direction as we used to know them.

The elevator or horizontal rudder as it was called was another new device worked out to meet another well-known difficulty, that of controlling the movement of the centre of pressure. The front part was fixed, and the back edge could be moved up and down. The effect was that in the neutral position the plane was flat, while a movement of the back edge gave it a curve upwards or downwards with an increasingly powerful action either way.

On trial, the 1900 machine did not come up to expectation. The resistance or drag of the planes was remarkably low, but the lift did not come nearly up to the figure deduced from Lilienthal's tables, upon which the machine had been designed. The lateral and fore and aft controls were very powerful, and were thought at this stage satisfactory.

The next year exactly the same type of machine and arrangement was followed, but the area was nearly doubled. It should be noted that the aspect ratio of both these gliders was little more than 3 to 1.

At first this 1901 machine could only be controlled with extreme difficulty, as it insisted on climbing steeply or diving. The full control had to be used to prevent an accident. The trouble was eventually traced to the big camber of the main planes causing excessive movements of the centre of pressure and stalling. After this was cured some good glides were obtained. Octave Chanute was stopping at the Wrights' camp and told them that they were well in advance of any other experimenters. Nevertheless, the Wrights themselves, judging from the trouble they had had with the centre of pressure movement and with certain unexplainable happenings on the lateral control, were at the end of these 1901 experiments considerably discouraged, as they were then beginning to appreciate how much there was to be done.

They saw that their experiments up to date, superficially successful as they had been, in real truth showed that they had no sound knowledge as to the lift, or the resistance or the centre of pressure movement of a curved plane, much less how such knowledge, if they had it, was to be applied to obtain a practical controllable aeroplane.

Fortunately, they were so deeply interested that they were unable to drop the subject and were sufficiently courageous to commit themselves to a lengthy programme of laboratory research on the characteristics of aerofoils.

This was the real starting point of their ultimate success.

In the 1916 Wilbur Wright lecture, Mr. Griffith Brewer has described something of the range and method of this experimental work.

During the years when I was in constant touch with the Wrights, we were more interested in the application of the results than in the method by which they had been obtained. To my regret I must confess that I did not pay to the details of this work the attention which I now realise was its due.

I saw parts of the 16in. channel apparatus lying about in corners and examined some of the little metal aerofoils tested. My recollection of these is that they were about 6in. by 2in., and that the method of measurement used was one of comparison against a variable area presented normally to the wind stream. The variable area consisted of a system of fingers of known dimensions which were arranged like a set of weights. A great deal of time and trouble was spent in getting the apparatus to function in a manner considered satisfactory, but once it was tuned up, tests were conducted with great rapidity, and as to accuracy we have the solid and undeniable fact that full scale design based on

this model work was entirely satisfactory and in accordance with expectation. I have been given by Wilbur or Orville hundreds of aerodynamic figures of all kinds—speeds, climbs and propeller revolutions, etc.—and have never known the estimated figure to have been more than 1 per cent. or 2 per cent. out from the measured result.

An immediate effect of the channel work was to be seen in the 1902 glider, whose plane had an aspect ratio of 6 and which had a fixed fin behind the planes. The gliding angle was found to be considerably better than the year before.

The first stage in the search for control had been the warping wings; the fixed fin of 1902 was the second stage and was the result of a study of the lift and drag figures obtained during the winter's work in the laboratory. From these figures they found an explanation for the occasional failures of the lateral control, when the lower wing continued to drop although its angle of incidence had been increased. What was of course happening was that the drag of that wing was increased and its speed decreased and that this loss of speed more than counteracted the increase of angle. By means of the affixed fin the Wrights expected to hold the wing up to its work and make the warp operate properly, and up to a certain point it did so.

The 1902 glider with the fixed fin undoubtedly was an improvement on the 1901 machine, but still it was not right and difficulties of control were occasionally experienced. When the machine side-slipped the fixed fin which was behind the wings tended to turn the machine about the vertical axis, speeding up the upper wing and slowing down the lower and so increasing the bank. This difficulty was eventually met by making the fin into an adjustable one, which could be turned to obtain the necessary pressure on the side towards the upper wing. It would appear probable that it was the actual movements of the aeroplane itself during the moments of instability which gave the clue to this, but it is hard to believe that the Wrights would have so quickly found a way through the bewildering tangle of these problems of control without the fundamental characteristics of an aerofoil to fall back upon.

When in the following year the first power machine was being designed they were able to go straight ahead without serious difficulty until they found themselves up against the design of the propeller. This proved to be a very serious obstacle and was not surmounted for several months.

The first power flights of December, 1903, were, apart from their great historical importance, really only a practical demonstration of the research work and technical development of the preceding three years.

During the next two years the experimental flying, apart from the mechanical troubles incidental to such work, was continually being held up by difficulties which could only be met by going back to the first principles and the characteristics of an aerofoil.

As an instance, they had trouble with instability on a turn which afterwards turned out to be due to stalling. The 1905 machine was flying with so small a margin of power that sometimes on a turn the angle of incidence of the lower wing tip would exceed the "critical angle" of lift and lateral control would be lost. As is well known serious accidents are still occurring from this cause.

It was eight or nine years before clear explanations of the phenomenon of the stalling of an aeroplane on the straight or on a turn were available. Certainly those who worked on the Public Safety and Accidents Committee in 1912 and 1914 will remember how inexplicable some of the accidents appeared to be, how widespread, even in 1913, was the lack of knowledge and understanding of this matter.

The first lesson to be learnt from the work of the Wright brothers is of the immense importance of fundamental research work in the laboratory.

Another very important lesson to be drawn is as to the value of the closest co-operation between laboratory and field work.

The first period was devoted to the study of all the available information and to thought about the essentials of the problem. Then came two periods of practical experiment in the field, the first short and the second of considerable length. This full-scale work gave the Wrights a good deal of experience and confidence in what they were doing, but above all made it clear to them that whether they liked it or not they had to go back again to the beginning and get down to the fundamental research, which was the only foundation on which a permanent structure could be built.

With data in their possession they returned to the application of it on the full scale with immediate success and from then on their progress was steady and continuous.

The reason why this research data obtained by the Wrights could not be given to the world was that their own financial situation and their obligations to the purchasers of their patents, in their opinion, compelled them to keep the data in their own possession.

It is not my wish to exaggerate the importance to the world's knowledge of aeronautical research, of the work of the Wright brothers, but it is my desire to lay the strongest emphasis on the lessons to be learnt therefrom, namely, that the whole basis of aeronautical progress rests upon genuine research in the laboratory, on the development of mathematical lines of attack and on full-scale research work in the field, and cannot possibly rest only or even mainly upon technical development.

Brilliant craftsmen and engineers though they were, it is certain that the Wrights would never have solved the problem of flight if they had not the fundamental data to guide them in times of difficulty.

Turning now to the immediate pre-war period, we find that aeronautical research in this country was put on a very solid foundation, and between the year 1909 and 1914 a very large amount of the necessary fundamental data was obtained at the National Physical Laboratory, at the Aircraft Factory, Farnborough, and by independent experimenters. Both the official establishments were generously subsidised and supported from national funds and all the necessary elaborate apparatus up to 7-foot wind channels were built, developed and worked at high pressure.

Before the outbreak of the war complete information was obtained and was available for designers as to the lift, drag and centre of pressure movement of a wide range of aerofoil sections, as well as for a large number of complete models.

Figures were available as to the actual pressures experienced over the upper and lower surfaces of the aerofoil.

The relations between model and full scale had reached a state of knowledge almost as far advanced as to-day.

Resistance of all exposed parts, such as bodies, struts, wires, wheels, etc., had been measured and were available for the designer. All the main problems of stability as regards the longitudinal movements had been investigated mathematically and experimentally and were considered to be satisfactorily solved. As regards the lateral and directional stability, the problems had been dealt with on mathematical lines and the lines of experimental investigation determined. The complete solutions to these problems have not yet been found.

The strength requirements and methods of stress calculation for an aeroplane had been settled and the load factors then decided upon are substantially the same to-day, although the increase in dimensions has introduced certain modifications. All the main facts as regards propeller design and manufacture were available.

Finally, practically all the vital features of this information had been checked by full-scale work and all the instruments really essential for the safety of the pilot had been designed and tested.

It is impossible at this date to read again the Advisory Committee reports up to the beginning of the war, as I have done in the preparation of this paper,

without being astonished at the wide range of the work and the accuracy of the deductions.

Research in the War Period.

It is frequently stated that aeronautical progress under the stimulus of war was rapid and continuous. This statement is only true to a limited degree. Progress in the art of flying itself, in the handling of aircraft by pilots was certainly both rapid and continuous, as also was the progress in methods of construction, materials and design from the engineering standpoint.

Great also was the progress in aircraft engines as regards the actual power, reliability, efficiency and reduction of weight. To this progress in engines is due almost entirely the tremendous increase in performance which was so marked a feature during the war.

In coming to a consideration of the progress in aerodynamics during this period, it is accurate to state that substantially there was none. Practically as much weight per horse-power could be carried in 1914 as in 1919 and as safely, and when one comes to think of it this was only to be expected, since during such urgent times efforts were concentrated on the technical development of what was known to be on sound lines rather than on researches, which were of necessity to some extent vague and indefinite and possibly of no immediate practical utility.

Research in the Post-War Period.

Since the war, money and the national effort as distinct from the individual effort put into research have steadily dwindled until it is safe to say that it is now far below the pre-war standard, when we had as well as the establishment supported by State fund, a large amount of work being done by private firms and individuals.

At the present date the few, extremely few, private firms who are left are so hard pressed to keep on their feet at all that they find it impossible to continue to devote their resources to anything outside the fulfilment of their definite orders.

As for private individuals they are, to the best of my knowledge, completely extinguished by the heavy hand of fate.

One of the most unfortunate results of the concentration during the war of all aeronautical matters into the hands of the State is that individual effort is now almost paralysed, and it is impossible for any one man, however broad-minded and far-sighted, to sympathise with all the wide aspects of aircraft research, particularly when continually harassed by orders to cut down his expenditure by so much per cent. as if he were a commercial traveller buying fireirons.

To all persons interested in sound aeronautical development it is only too evident that the importance of fundamental research to real progress in aircraft design is not grasped by those in authority in this country. Even the word research itself is very imperfectly understood.

The initial problems are solved and technical development has gone far during the last ten years, but if the flying machine is to be of real benefit to the world in peace and to be a real means of rapid communication and of increase of friendliness among peoples, then big advances are necessary. Such advances are only to be obtained by laborious and systematic researches into the problems we can see dimly as well as those we can see more clearly immediately in front of us.

The immediate problems are those directed towards the improvement of the present type of aeroplane so as to increase its safety of operation. The main obvious defects are, that its minimum flying speed is too great and that the stability and controllability round about this speed are insufficient.

It is foolish to put the responsibility for accidents on the engine and to say that all that is necessary is so to improve the reliability of engine and installation that breakdowns cannot occur. Certainly the majority of interruptions are trace-

able to some breakdown in the power plant in which greater experience and care will undoubtedly make big improvements; but it can never be the case that mechanical breakdowns are absolutely impossible and it is essential that emergency landings can be accomplished without danger to the passengers or without even anxiety to the pilot. This is far from being the case at the present time and it is a mistake to attempt to conceal it.

To the immediate future of civil aeronautics the importance of safety is paramount and supreme over all other considerations, and the efforts which are now being made in the laboratory and on full scale to improve stability and controllability round about stalling speed are all to the good. The other main problem, namely, that of decreasing the dangers at the landing itself, cannot be attacked in an adequate manner until funds are available. This problem has two branches, one a decrease in the minimum flying speed while keeping the top speed about the same, and the other, an increase in the strength and efficiency of the landing mechanism itself. These are the immediate needs of the aeroplane itself, apart from the power plant. To go into the improvements which are immediately necessary in the latter would be really outside the scope of this paper, and in my opinion the main defects are at the moment in the aeroplane rather than in the engine.

Further off we can see the big advances which will be possible when we have a real understanding of the action of an aerofoil in the air, and how and why the air flows round it and gives it lift and drag. A brilliant experiment may show, and indeed has shown, that we are on the outskirts of knowledge, but before we can attain the citadel we must give encouragement and adequate support to the men who are devoting themselves to fundamental research of this character.

It is conceivable that lines of advance may then reveal themselves which would make it worth while to attempt even the design of a helicopter, but let us recognise that the present time is one requiring research work into the fundamental problems which must be solved before the world at large can reap the benefits of the wonderful achievement of Wilbur Wright and his brother.

TABLE A.

WRIGHT AEROPLANES 1910.

	Standard Machine.	Racing Machine.
Span	39ft.	20ft. 6in.
Chord	6ft. 3in.	3ft. 6in.
Gap	5ft. 6in.	3ft. 6in.
Area—main planes	475 sq. ft.	150 sq. ft.
Engine	35 h.p. (4-cyl.)	60 h.p. (8-cyl.)
Total weight	1,320lbs.	900lbs.
Loading per h.p.	37.5lbs.	15lbs.
Loading per sq. ft.	2.75lbs.	6lbs.
Speed	38 m.p.h.	75 m.p.h.
Weight Details.		
Structure	475lbs = 36%	250lbs. = 28%
Engine rad. and water ...	285) 12lbs. per	361) 7½lbs. per
*Propellers, chains, etc. ...	132) h.p.	87) h.p.
Fuel and tank	84 (10 gal.)	74 (9 gal.)
Crew	344 (2)	128 (1)
Total	1,320lbs.	900lbs.

* Same type of propeller was used for both machines.

TABLE B.

DATA AS TO GLIDERS.

Year.	Span.	Chord.	Area.	Weight. Empty	Weight. Loaded	Angle of Glide.	Loading lbs. per sq. ft.
1900	18ft.	5ft.	165 sq. ft.	50lbs.	190lbs.	8° to 9°	1.15
1901	22ft.	7ft.	308 ,,	100lbs.	240lbs.	7° to 8°	.78
1902	52ft.	5ft.	305 ,,	120lbs.	260lbs.	6° to 7°	.82
1911	32ft.	5ft.	305 ,,	145lbs.	310lbs.	6°	1.00

HELICOPTERS.

BY JOHN CASE, M.A., F.R.AE.S.

Summary.

The present article is an account of some calculations on helicopters.

Airscrews have been calculated for different conditions according to two theories: (1) the multiplane interference theory; (2) Glauert's vortex theory. According to both there should be no difficulty in designing a screw to give a good lift at a reasonable rate of climb, and the ceiling should also be quite good. When we consider the speed of falling, with the screws free-wheeling, the two theories give widely different results, and the practicability of the helicopter depends largely upon this question being settled. Simple airscrew theory shows that at least moderate speeds should be obtainable by inclining the airscrew axis. For many reasons it seems desirable that the screws should have at least four blades; gyroscopic couples on the whole machine are eliminated; the forces are widely fluctuating during forward motion with only two blades; the stability derivatives and equations are simplified; but the aerodynamic efficiency will be impaired. In general the stability equation is of the tenth degree, and the lateral and longitudinal stabilities are not separable when the machine, in a state of steady motion in a straight line, receives an asymmetrical disturbance.

The following notes are the results of an attempt to investigate the theoretical possibilities of the helicopter, and generally to develop some branches of the theory of helicopters. In this country extremely little work on the subject seems to have been published, and the only experiments I have been able to find are those given in the Report of the Advisory Committee for 1917-18 (Vol. II.). Several articles have appeared in "L'Aérophile" from time to time, notably by Lamé, Touissaint and Margoulis, and some experimental work has been done by Eiffel. But it is extremely difficult to find adequate experimental results with which to compare any theory; for instance it is very rare to find results of tests on airscrews working under helicopteral conditions, and also the aerodynamic data of the aerofoil sections used. In the matter of stability I do not know of any experimental work at all.

I shall give first the results of my investigations into airscrews for helicopters, and then proceed to the consideration of the dynamics of helicopter flight and the development of the stability equations.

1. Airscrews for Helicopters.

In setting out to calculate the characteristics of the supporting screws of a helicopter we must consider what method to employ, for evidently the simple blade-element theory, without corrections, cannot be expected to give reliable results when the airscrew is at rest axially. The two most modern theories of the airscrew are the multiplane interference method developed by R. McKinnon Wood,* and the vortex theory given by H. Glauert.† Both these methods seem to give good results when applied to tests on the type of airscrews used for aeroplanes. In using the multiplane interference method we may estimate the corrections from the experimental data given in R. and M. 639, or calculate them by the vortex theory of aerofoils as suggested by H. Glauert.‡ For the helicopter we must be able to calculate the properties of an airscrew when working under the condition of no advance in the direction of the axis, and the first step I have taken is to compare calculated and experimental results. Unfortunately very little can be done in this direction.

* See R. & M. 639. † See R. & M. 786. ‡ See R. & M. 752.

Taking first the helicopter screws described in the A.R.C. Report for 1917-18, no direct experimental data exist for the properties of the cross sections of the blades and the best we can do is to plot probable lift and drag curves for the sections according to their camber. This refers only to the blades having a flat under-surface.

In the propeller considered the pitch angle is constant along the blade. First the thrust was calculated with the blades set at 2 deg. to the plane of rotation, the corrections being estimated from the experimental data given in R. and M. 639. It was found that a screw 40ft. dia. running at 275 r.p.m. should give a thrust of 2,000 pounds per blade; according to the test on the complete model airscrew the thrust should be 400lbs. per blade. Next the screw was calculated under the same conditions, by the same method, except that the corrections were calculated by the vortex theory of aerofoils; this led to a thrust of 370lbs.§ per blade. With the blades set at 6 deg. and running at 144 r.p.m. the same process gave a thrust of 425lbs.§ per blade, the value deduced from the model test being 400lbs. in each case. Thus the agreement between the calculated and experimental results was fairly good when the multiplane effect was calculated by the vortex theory.

The next example chosen for calculation was the propeller used by the N.P.L. for pressure distribution experiments and described in R. and M. 681. It is not possible to check the multiplane interference method for no advance with this screw, as the angles are too large, but I have used it to check the vortex theory of airscrews. Fortunately, for this screw we have tests on the sections to which the blades were cut. At 15 r.p.s. the calculated thrust is 14.4lbs. on the model, and the observed thrust was 13.2lbs.; the calculated torque is 3.08lbs. ft., and the observed torque was 3.25lbs. ft.

From these figures it appears that we might expect to obtain reasonable results for airscrews working under helicopteral conditions either by Glauert's vortex theory of airscrews, or by the multiplane interference method, provided we calculate the corrections by the vortex theory of aerofoils. At the same time it must be remembered that the experimental evidence is very slight. Of the two methods the vortex theory of airscrews is the more purely logical, and involves no empiricism, while the question of aspect ratio must always make the multiplane method rather doubtful. When I started on this work I was not aware of Glauert's paper, and worked entirely with the multiplane method, calculating the corrections by the vortex aerofoil theory. Since then I have done most of the calculations again by his method, and there is considerable difference between the results. For helicopter screws hovering or climbing, Glauert's theory gives more hopeful results than McKinnon Wood's multiplane method. but the converse is the case when we deal with a helicopter falling with the screws free to rotate as windmills.

2. H. Glauert's Airscrew Theory.

As far as concerns airscrews used on aeroplanes, this is fully set out in R. and M. 786, and will not be described here, except to say that it differs very little from the common inflow theory in method, and that the fundamental principles of it are that the theoretical inflow factor $\frac{1}{2}$ is used, and the aerofoil characteristics employed are those of the monoplane of infinite aspect ratio. These are obtained from the characteristics of the finite monoplane by the method set out in R. and M. 723.

The manner of dealing with the case of $V=O$, and the case of the helicopter descending with blades reversed is not contained in R. and M. 786, but has been given to me since by Mr. Glauert, and is given here.

§ These figures were found by calculating for small values of V/nD and extrapolating, as the method breaks down when V is zero.

3. Helicopter Screw Hovering.

The following notation is used :—

N = number of blades.
D = diameter of screw $= 2R$.
B = blade width at radius r.
k_L = lift coefficient for aerofoil, of infinite aspect ratio, of the same section as the airscrew blade at radius r.
k_D = drag coefficient of ditto.
θ = pitch angle of section.
α = angle of incidence.
$\gamma = \tan^{-1}(k_D/k_L)$.
$\Omega = 2\pi n$ = speed of rotation (radians per second).
$a''r\Omega$ = axial velocity of air through the propeller disc at radius r.
$a'\Omega$ = rotational velocity of the air in passing through the propeller disc.

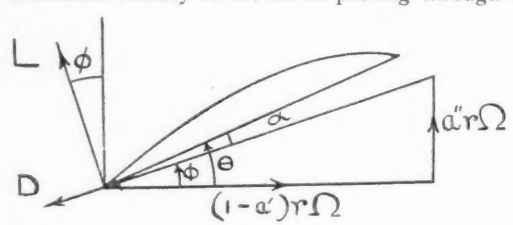


FIG. 1.

Then it can be shown that

$$\phi = \theta - \alpha \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$
$$\tan\phi = a''/(1 - a') \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$
$$a' \tan^2\phi = a'' F'' \tan(\phi + \gamma) \quad . \quad . \quad . \quad . \quad . \quad (3)$$
$$a''^2 (1 - F'' \cot^2\phi) - 2\delta' a'^2 = 0 \quad . \quad . \quad . \quad . \quad (4)$$

where

$$F'' = \{ k_L \cos(\phi + \gamma) \} / (S \cos\gamma \sin^2\phi) \quad . \quad . \quad . \quad (5)$$
$$S = 4\pi r/NB \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

and δ' is the quantity referred to in R. and M. 786, and may be taken as zero without serious error. Taking arbitrary values of α, θ is given, and so ϕ is obtained from (1); F'' is then calculated from (5) and (6), whilst (2) and (3) give a' and a''. All the quantities are now found, and it remains that equation (4) must be satisfied; by plotting the left-hand side against α we find the value of α which makes this zero. All the equations are then satisfied.

The thrust (dT) and torque (dQ) of the element are given by

$$dT = 4\pi r^3 \rho \Omega^2 (1 - a')^2 F'' dr \quad . \quad . \quad . \quad . \quad (7)$$
$$dQ = r \tan(\phi + \gamma)\, dT \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

If we write, as is usual,

$$T = \rho n^2 D^4 k_T$$
$$Q = \rho n^2 D^5 k_Q,$$

we have

$$k_T = 2\pi^3 \int_0^1 (1 - a')^2 F'' x^3 dx \quad . \quad . \quad . \quad . \quad (9)$$

$$k_Q = \pi^3 \int_0^1 (1 - a')^2 F'' \tan(\phi + \gamma)\, x^4 dx \quad . \quad . \quad (10)$$

where

$$x = r/R = 2r/D.$$

Thus, when plotting the left-hand side of equation (4) against α, it is advisable to plot $(1 - a')^2 F''$ so that the value of this quantity when equation (4) is satisfied is easily found. This is repeated for each cross section of the blade, and k_T and k_Q are then obtained by integration along the blade.

4. Descent with Screws Free.

If a helicopter is to descend safely in the event of engine failure, it must be possible to disengage the screws from the engines and to reverse the blades to prevent stoppage followed by rotation in the opposite direction. This case is calculated as follows :—Neglecting inflow the conditions which obtain at a blade

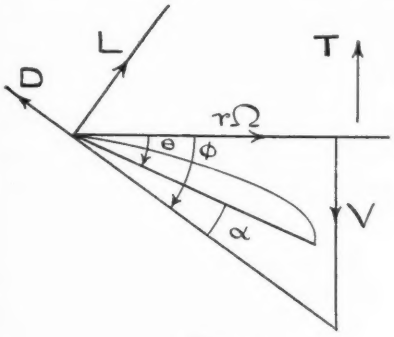


FIG. 2.

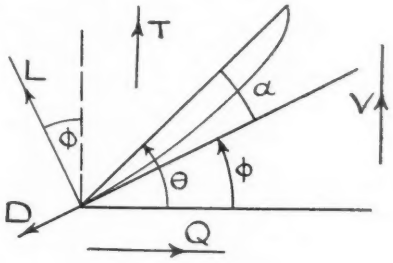


FIG. 3.

element are as shown in Fig. 2. If we still consider θ and ϕ positive, α will be negative, in the case shown, and k_L will be positive; if α be positive (*i.e.*, $\phi < \theta$) and numerically greater than the no-lift angle, k_L will be negative. Evidently we can bring this case into line with the standard case as shown in Fig. 3, *i.e.*, if we change the signs of both α and k_L in the aerofoil characteristics. Then a negative value of T and a positive value of V correspond with the helicopter falling against an upward thrust, and a negative value of Q corresponds with a braking couple applied to the shaft.

The notation is the same as in the previous case, with the following additions or modifications :—

V = axial velocity of screw

aV = increase of relative velocity of air in passing through the propeller disc.

$$F = \{ k_L \cos(\phi + \gamma) \} / \{ S \cos\gamma \sin^2\phi \} \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

and it should be noted that, when k_D/k_L is numerically small, γ is in the neighbourhood of 180° not 0°.

Taking arbitrary values of α, F is calculated from (11), and then

$$J = V/nD = \pi x \tan\phi_0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

where

$$\tan\phi_0 = \{ (1 - F) \tan\phi \} / \{ 1 + F \tan\phi \tan(\phi + \gamma) \} \quad . \quad . \quad . \quad (13)$$

We also have

$$dk_T/dx = J^2 \pi x F/(1 - F)^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

$$dk_Q/dx = -(dk_T/dx)(x/2) \tan(\phi + \gamma) \quad . \quad . \quad . \quad . \quad . \quad (15)$$

where, as before, $x = r/R$.

Thus we can plot dk_T/dx and dk_Q/dx against J for each section of the blade, and hence plot them along the blade for a given value of J, and, by integration, find k_T and k_Q. Finally we can plot k_Q and k_T against J. If there is no applied

couple k_Q must be zero. This gives the value of J at which the helicopter will fall. Neglecting other resistances we must have

$$T = \rho n^2 D^4 k_T = W$$

where W is the weight supported. Hence both n and V are determined.

5. Numerical Investigation.

In investigating the properties of airscrews for helicopters I have used the blade shown in Fig. 4.

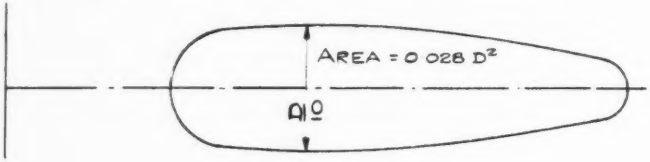


FIG. 4.

The sections are series B in R. and M. 362, and the numbers used here to denote the sections are the same as in R. and M. 362. For the rest, the blade shape is defined as follows :—

Section	A	B	C	D	E	F
Aerofoil number ...	7	6	5	4	3	2
Camber	0.2	0.16	0.12	0.10	0.09	0.08
r/R	0.34	0.48	0.66	0.76	0.83	0.95
B/B_0	0.91	1.00	0.935	0.82	0.73	0.48

where B is the blade width at any section, and B_0 is the maximum blade width. The characteristics of the sections for infinite aspect ratio are shown in Fig. 5.

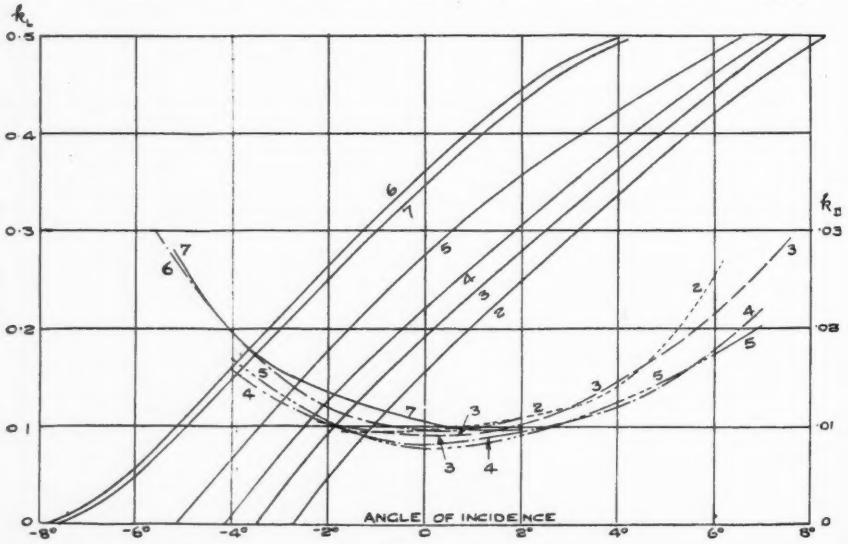


FIG. 5.

6. Helicopter Hovering.

The first calculations were made to determine the probably best ratio of pitch to diameter to obtain the greatest lift per horse-power when the machine is

stationary in the air, and to see what lift may be expected under these conditions. The results are given in Fig. 6, and Table I.

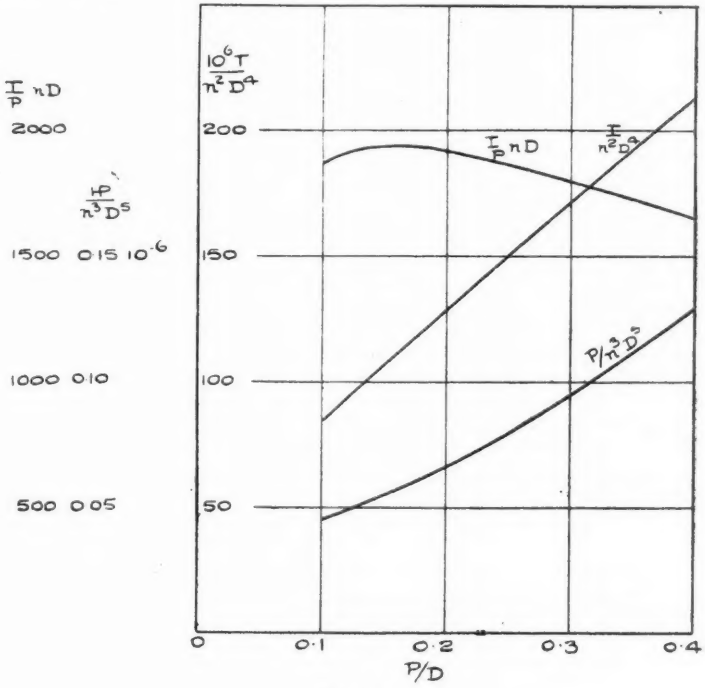


FIG. 6.

TABLE I.

HELICOPTER SCREW HOVERING—TWO-BLADE AIRSCREW.

Pitch/Dia.	0.1	0.2	0.3	0.4
$T/n\,D^4$ ...	85.3	128	170	213.10^{-6}
$Q/n^2\,D^5$ ...	4.0	5.89	8.30	$11.3.10^{-6}$
$P/n^3\,D^5$ ...	0.0456	0.0672	0.0948	$0.129.10^{-6}$
$nD\,T/P$ ...	1870	1910	1790	1650

T=thrust (lbs.), Q=torque (lbs. ft.), P=horse-power at standard density; n=revs. per sec., D= dia (ft.).

It appears from this that the best pitch-dia. ratio for hovering will be about 0.2. As an example of the meaning of these figures it will be seen that a two-blade screw 40ft. dia. running at 60 r.p.m. would give a total lift of about 330lbs., at the rate of 48lbs. per horse-power. There is evidently no difficulty, according to these figures, in designing a helicopter to support itself in still air.

7. Helicopter Climbing.

The thrust per horse-power obtainable from a helicopter screw which is at rest axially is obviously no criterion of the feasibility of the helicopter, for the thrust will certainly be less when the machine is climbing. To ascertain the effect of the rate of climb I have taken a screw as described above, with a pitch/dia. ratio of 0.3, and having two blades. The general results of the calculations are given in Table II. and Fig. 7.

TABLE II.

HELICOPTER SCREW CLIMBING. PITCH/DIA. = 0.3.

$V/n\,D$	0	0.20	0.25	0.30	0.35	
k_T	0.0716	0.051	0.0434	0.0346	0.026	
k_Q	0.0035	0.00338	0.0032	0.0030	0.00278	
$T.10^6/n^2\,D^4$	170	121	103	82	61.5	Calculated by vortex theory of airscrews.
$Q.10^6/n^2\,D^5$	8.3	8.0	7.6	7.1	6.6	
$P.10^6/n^3\,D^5$	0.0948	0.0913	0.0867	0.081	0.0754	
$nD\ T/P$	1800	1330	1190	1010	815	
$T.10^6/n^2\,D^4$		82.4	66	50	36	Calculated by multiplane theory with calculated corrections.
$P.10^6/n^3\,D^5$		0.0774	0.074	0.070	0.065	
$nD\ T/P$		1070	870	720	590	

For comparison the results of the two methods of calculation are given, and it will be seen there is considerable difference between them. In Fig. 7 only the results found by the vortex airscrew theory are shown.

Now let us suppose it is required to carry a nett weight of 1,000lbs., exclusive of the airscrew surfaces, and let us take the weight of these as 1.75lbs. per square foot, and let the rate of climb at ground level be 350ft. per minute. We consider the question of the number of propellers used, and obtain the following results :—

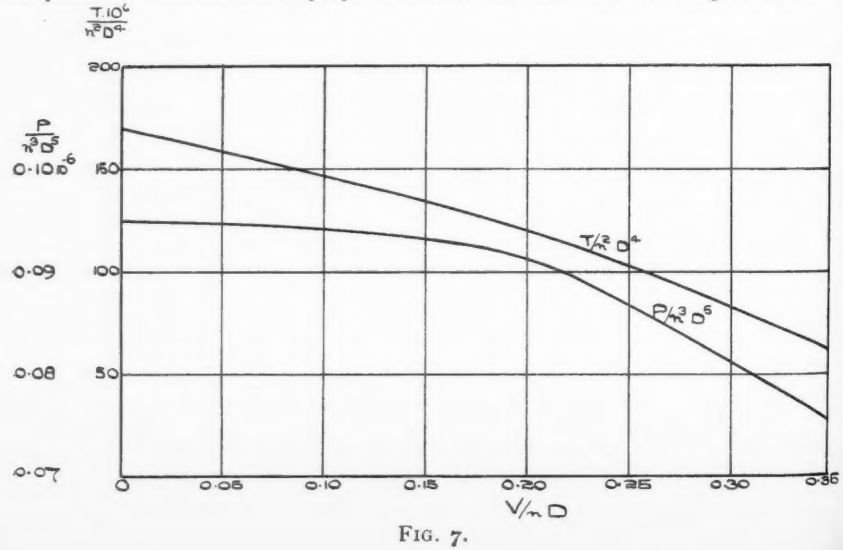


FIG. 7.

TABLE III.

Diameter 25ft., two blades.

Number of screws	2	4	6
Revs. per sec.	3.08	2.34	2.04
Nett lift per h.p.	18.5	21.7	20.9
Total lift per h.p.	20.7	27.0	28.4

Diameter 40ft.

Revs. per sec.	1.32	1.06	0.97
Nett lift per h.p.	22.2	20.8	17.6
Total lift per h.p.	29.4	33.7	33.8

These figures were obtained by the vortex airscrew theory, and are extremely hopeful. The figures found by the multiplane theory are not quite so good, but are still very promising, thus with two screws 40ft. dia. a nett lift of 14.3lbs. per h.p. is given, and this method of calculation does not show so much difference between the results of using two, four, or six propellers. The figures shown in Table III. show a great advantage in using several screws, but this might easily be cancelled by the extra complication and weight of gearing necessary, and it must also be remembered that these calculations take no account of the interference between the screws, which will be almost inevitable.

8. Next let us consider the effect of rate of climb on the power required. Again, suppose the weight to be lifted is 1,000lbs. exclusive of the lifting surfaces, and that these weigh 1.75lbs. per sq. ft. Let us use two 2-blade screws, each 40ft. dia., with a pitch/dia. ratio of 0.3. The power required for different rates of climb at ground level is shown in Fig. 8, on the assumption of given total weight (in this case about 1,310lbs. with the airscrews).

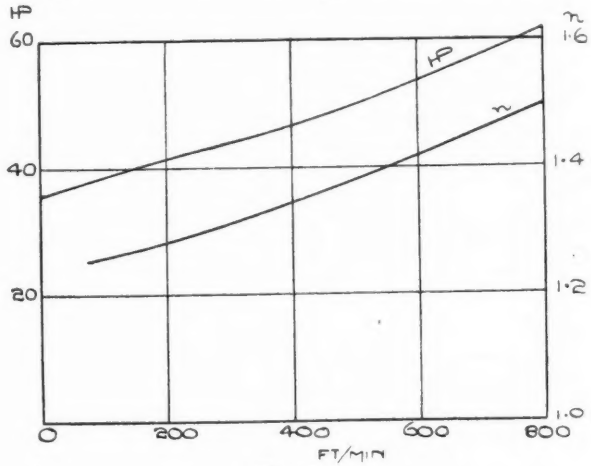


FIG. 8.

Now suppose our machine fitted with an engine which will give a maximum propeller horse-power of 60 at ground level, at 90 r.p.m., and examine what will be the ceiling. Such an engine will give a climb of 740ft. per minute at ground level. The speed of rotation necessary to hold the machine in the air will be proportional to $\sigma^{\frac{1}{2}}$, and the power required will vary as $\sigma^{3/2}$, where σ is the relative density. If we use the height factor for engine horse-power given by Bairstow,* and assume the engine power at a given height to be proportional to

* "Applied Aerodynamics," Fig. 205b.

the speed, we find that the ceiling will be about 8,000ft. If a supercharger be fitted the ceiling will be 14,000ft. if 90 r.p.m. represents the maximum permissible speed of the engine. If the engine can run up to a propeller speed of 100 r.p.m., the ceiling will be increased to about 20,000ft.

9. Descent with the Screws Free.

I shall suppose here that the screws are designed to get the best results under this condition, regardless of their efficiency for climbing purposes. The method of calculation has been described above.

Taking first a pitch/dia. ratio of 0.3, it will be found that, according to the vortex theory, Q is zero when $V/nD=0.32$, and that then the thrust coefficient k_T is 0.03, so that, at standard density $T=690\ V^2\ D^2\ 10^{-6}$lbs., and the value found by the multiplane theory is not very different. When the pitch/dia. ratio is reduced to 0.2 the thrust when the torque is zero is increased, slightly, to $720\ V^2\ D^2\ 10^{-6}$. If the pitch is reduced to 0.1 D the airflow appears to be very uncertain and it is difficult to interpret the theoretical figures found by the vortex method. An improvement is effected by using a blade in which all the sections are aerofoil section 7 (see above); with a pitch equal to 0.3 D, we find $T=810\ V^2\ D^2\ 10^{-6}$. With this value for T, a screw 40ft. dia. would exert a braking force of 500lbs. when falling at 20ft. per sec. In this type of screw the airflow seems to become uncertain when the pitch is reduced to 0.2 D. The thrust exerted by two-blade screws of this type, with a pitch=0.3 D, falling at various speeds, is shown in Fig. 9.

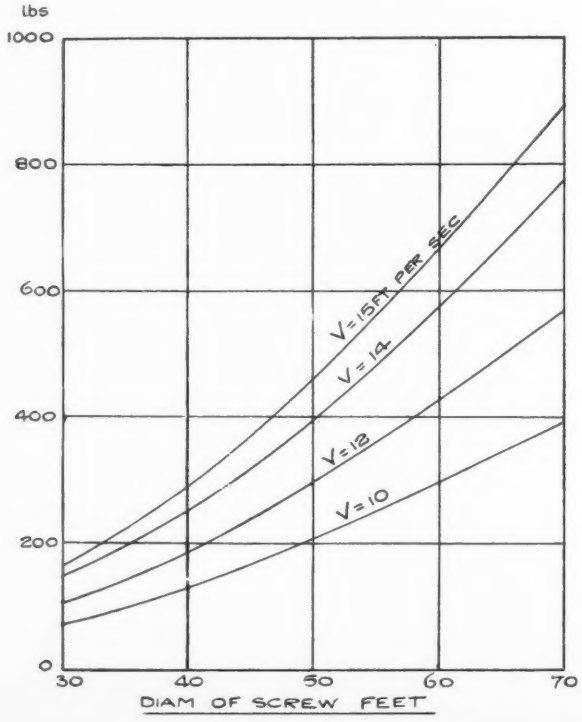


FIG. 9.

From this it will be seen that it would take six such screws, each 60ft. dia., to support a weight of 1,200lbs. exclusive of their own weight, when falling at 12ft. per sec., if each screw weighs 225lbs.

The figures obtained by the multiplane method, calculating the interference effect by the vortex theory of aerofoils, promise a much more easy solution of the problem of safe descent with the screws free, for according to this method of calculation six propellers 40ft. dia. would support 2,800lbs. exclusive of themselves, when falling at 13.5ft. per sec. The results of the two methods of calculation are summarised in Table IV. and Fig. 10.

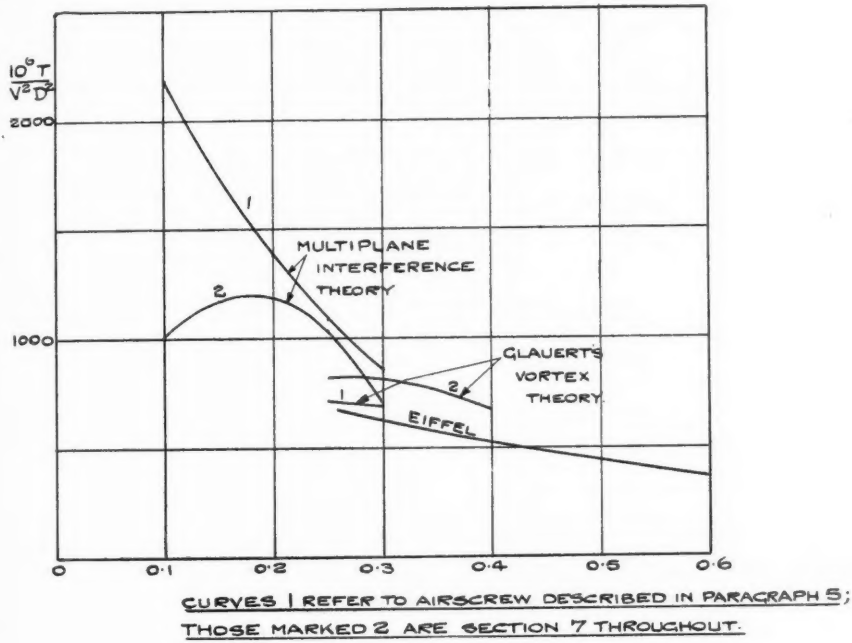


FIG. 10.

TABLE IV.

Standard Blade as above.				All Blade Sections are No. 7.				
Pitch/Dia.	0.1	0.2	0.3	0.1	0.2	0.25	0.3	0.4
V/nD	0.2	0.265	0.30	0.27	0.31			
$10^6T/n^2\,D^4$	79.2	85	63.6	159	138			
$16^6T\,V^2/D^2$	990	1210	710	2180	1432			
V/nD	—	0.365	0.32		—	0.42	0.375	0.37
$10^6T/n^2\,D^4$	—	95	71.1		—	144	113	93.5
$10^6T/V^2\,D^2$	—	720	690		—	815	810	685

The table shows the values of the various quantities when $Q=O$; the first set of figures was obtained by the multiplane method, the lower set by the vortex airscrew theory.

Taking the best results of each, the multiplane theory gives a lift of more than $2\frac{1}{2}$ times that given by the vortex theory for the same speed of falling.

It is worth noting perhaps that the drag of a fixed airscrew of this shape, of R.A.F.6 section, would be $93\ V^2\ D^2\ 10^{-6}$.

10. Forward Motion by Inclination of Screw Axis.

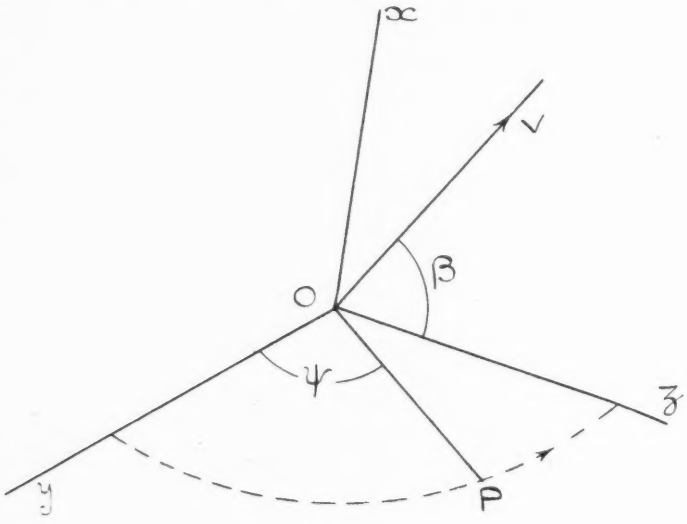


FIG. 11.

Referring to Fig. 11, let the axis of x be directed along the airscrew axis, and let the axis of y be at right-angles to the plane containing Ox and the direction of motion of the centre of the screw; Oz is at right-angles to Ox and Oy.

Let V be the velocity along the flight path.

β = the angle VOz.

The component velocities of the blade element P are

$V \sin \beta$ . . . . along Ox.
$V \cos \beta \cos \psi + \Omega r$ at right-angles to OP and Ox.
$V \cos \beta \sin \psi$ . . . . along OP.

We neglect the effect of the last of these.

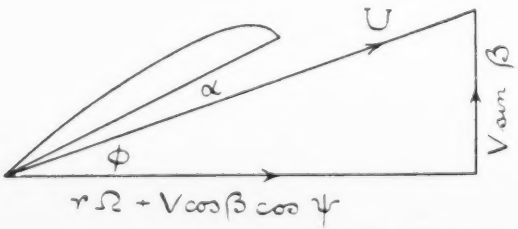


FIG. 12.

Let U be the resultant velocity of the wind relative to the blade element, and making an angle ϕ with the plane of motion.

Let X, Y, Z be the total force on the blade in the position ψ.

Let Q be the corresponding torque.

Let $\bar{X}$, $\bar{Y}$, $\bar{Z}$, $\bar{Q}$ be the mean value of these quantities taken round a whole revolution.

Then, with the simple blade-element theory, we have

$$X=\rho B_0 D^3n^2\int_0^1 B/B_0\ (U/nD)^2\ (k_L\cos\phi—k_D\sin\phi)\ d\xi.$$

$$Y=\rho B_0 D^3n^2\int_0^1 B/B_0\ (U/nD)^2\ (k_L\sin\phi+k_D\cos\phi)\sin\psi\ d\xi.$$

$$Z=\rho B_0 D^3n^2\int_0^1 B/B_0\ (U/nD)^2\ (k_L\sin\phi+k_D\cos\phi)\cos\psi\ d\xi.$$

$$Q=\tfrac{1}{2}\rho B_0 D^4n^2\int_0^1 B/B_0\ (U/nD)^2\ (k_L\sin\phi+k_D\cos\phi)\ \zeta\ d\xi.$$

and

$$\bar{X}=1/2\pi\int_{2\pi}^{0} X\ d\psi \qquad \bar{Y}=1/2\pi\int_0^{2\pi} Y\ d\psi \qquad \bar{Z}=1/2\pi\int_0^{2\pi} Z\ d\psi \qquad \bar{Q}=1/2\pi\int_0^{2\pi} Q\ d\psi$$

where

$\xi=r/R=2r/D$

B = blade width at radius r

B_0 = maximum blade width

$$U^2=(2\pi rn+V\cos\beta\cos\psi)^2+V^2\sin^2\beta$$

$$\tan\phi=\{\ (V/nD)\sin\beta\ \}\ \{\ (V/nD)\cos\beta\cos\psi+\pi\xi\ \}$$

It would seem that this is the only calculation we can do at present for an airscrew working under these conditions, for all our vortex theories presuppose small angles of incidence, whereas in the case we shall examine presently the angle of incidence varies from $+9°$ to $-144°$.

11. Before making a numerical analysis of a screw let us investigate the magnitudes of the various quantities. We shall write

$$T=\rho n^2D^4k_T$$
$$Z=\rho n^2D^5k_Z$$

Experiments made on a propeller with its axis at right angles to the wind ("Aeronautical Research Committee," 1916-17, Vol. I., p. 293) show that the thrust is very little affected by the side wind up to the greatest value of V/nD (2.2) used in the experiments.

Also we can write,* when β is 90°,

$$k_T=k_{TO}-b\ (V/n)^2$$

where

$$k_{TO}=4/3K$$

and

$$b=1/KP_m^2$$

* See R. & M. 474 or 577.

K being a constant of the screw such that $Kk_T=1$ when $V/nP_m=\frac{1}{2}$. In view of the experiments just cited we shall suppose that this holds for the inclined screw if we write $V \sin \beta$ for V. We then have

$$T=\rho n^2 D^4 \{ k_{T0} - b (V/n)^2 \sin^2 \beta \}$$
$$= T_0 - \rho b V^2 D^4 \sin^2 \beta.$$

T_0 being the value of T when β is zero.

The propulsive force in the direction of motion is

$$T \sin \alpha = (T_0 - \rho b V^2 D^4 \sin^2 \beta) \sin \beta.$$

This is a maximum when

$$T_0 - 3\rho b V^2 D^4 \sin^2 \beta = 0$$

which gives

$$\sin \beta = \sqrt{(T_0/3\rho b V^2 D^4)} = \sqrt{(K k_{T0} n^2 P_m{}^2)/3V^2}$$

i.e.,

$$\sin \beta = (2/3)(nP_m/V)$$

Now consider the screw whose characteristics are shown in Fig. 7. The thrust is zero when $V/nD=0.5$ about. Suppose $D=40$, then $P_m=20$. If $n=1.5$ and $V=150$ we find $\sin \alpha=0.13$, or $\alpha=$ about 8°.

12. Let us then take for an example

$$\text{Pitch} = 0.3D$$
$$V/nD = 2.0$$
$$\beta = 10°$$

The only aerofoil section of which we have knowledge for all angles of incidence is R.A.F. 6. We find, if $B_0=0.1\ D$.

$$\left.\begin{aligned} \bar{X} &= 84.10^{-6} n^2 D^4 \\ \bar{Z} &= 13.10^{-6} n^2 D^4 \\ Q &= 4.6.10^{-6} n^2 D^5 \end{aligned}\right\} \text{per blade}$$

at standard density.

The lift is

$$L = \bar{X} \cos \alpha + \bar{Z} \sin \alpha = 84.8.10^{-6} n^2 D^4$$

The drag is

$$D = Z \cos \alpha = 12.8.10^{-6} n^2 D^4$$

The propulsive force is

$$F = \bar{X} \sin \alpha = 14.6.10^{-6} n^2 D^4$$

Thus the nett propulsive force is $1.8.10^{-6} n^2 D^4$. If $D=40$ and $n=1.46$, this only amounts to 7lbs. per blade.

No improvement is obtained by increasing the pitch to $0.4D$. Better results, however, are obtained by making $\alpha=5°$ all along the blade when $\psi=0$. We then find

$$\text{Lift per blade} = 107.10^{-6} n^2 D^4$$
$$\text{Drag per blade} = 13.8.10^{-6} n^2 D^4$$
$$\text{Propulsive force per blade} = 18.4.10^{-6} n^2 D^4$$
$$\text{Lift/drag} = 7.75.$$

With two screws 40ft. diameter, each with two blades, running at 1.1 r.p.s., we find a nett propulsive force of 14lbs. per blade, or a total of 56lbs., the forward speed being 86ft. per sec.

Now the drag of B.E.2E. body (R. and M. 440) is 57lbs. at 100ft. per sec., so that if these figures are realised it should be quite possible to obtain at least moderate forward speeds by this means.

13. Periodic Nature of the Forces in Non-Axial Motion of the Screw.

Since the angle of incidence and resultant velocity of each blade element varies throughout a revolution when the machine has forward motion, the thrust, torque, etc., on each blade will be periodic with a frequency equal to the speed of rotation of the airscrew. This is shown in Fig. 13 for the force X along the

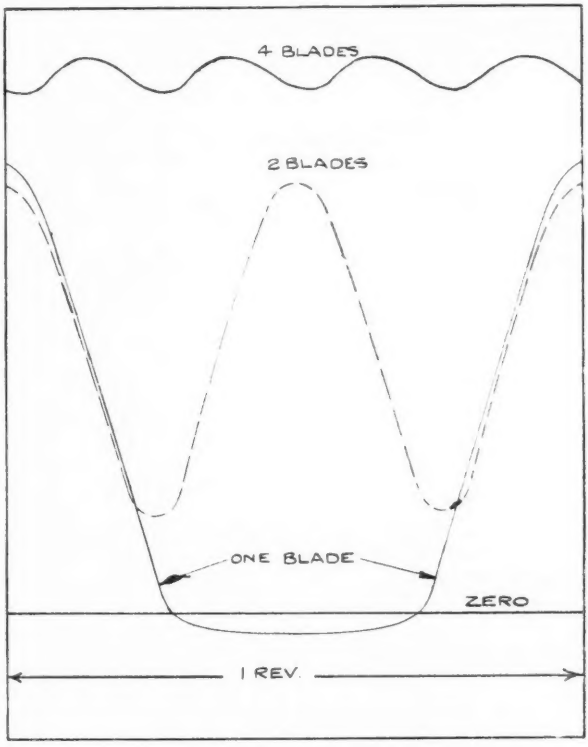


FIG. 13.

airscrew axis, and the quantities Z and Q vary in a similar manner. The diagram refers to the blade (R.A.F. 6 section) calculated above. It will be seen from this that it will be almost essential to use four-bladed propellers in order to obtain anything like a uniform thrust and torque.

13a. Fixing the Screw Form.

We have considered each problem of screw design as if it were quite independent. It remains to be seen whether it will be best to design the screw for climb, hovering, forward motion, or descent. The above figures suggest that it will probably be necessary to design the screws to ensure safe descent with the screws running free. If we have a true screw then, when the blades are reversed for climb or hovering motion we shall have a bastard shape. Possibly it will prove best to use flat instead of helicoidal blades, but it looks as if the chief difficulty will be to design screws which will ensure safe descent in the event of engine failure. This matter I shall hope to treat in a subsequent paper.

14. Gliding Down an Inclined Path with Screws Free.

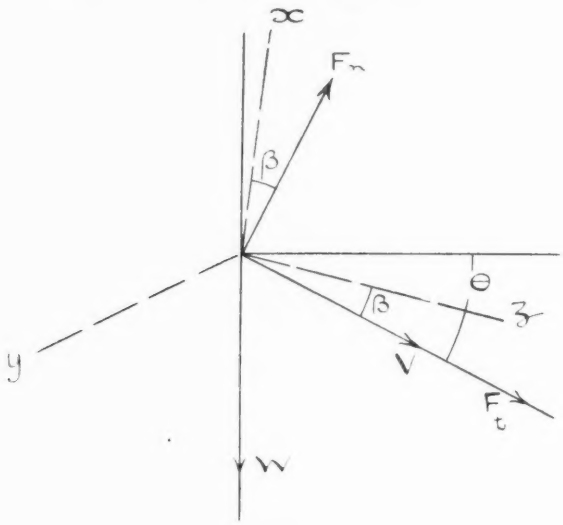


FIG. 14.

Let θ = inclination of flight path to the horizon.
Let F_n and F_t be the components of the forces on the airscrew normal and tangential to the flight path.
W = the total weight.

Then

$$F_n \cos\theta - F_t \sin\theta = W$$
$$F_n \sin\theta + F_t \cos\theta = O$$
$$\therefore \tan\theta = -F_t/F_n$$

TABLE V.

	V/nD	0	1	2	3	4	5
$10^6 F_t/n^2D^4$	$\beta = 0$	0	−49	−156	−350	−725	−1130
	$\beta = 15°$	20	−77	−220	−490	−960	−1470
	$\beta = 30°$	65	−136	−322	−680	−1280	
	$\beta = 60°$	120	−196	−425	−850	−1740	
	$\beta = 90°$	147	−234	−460	−970	−1890	
$10^6 F_n/n^2D^4$	$\beta = 0$	−150	−238	−268	−245	−170	0
	$\beta = 15°$	−150	−196	−180	−38	+265	755
	$\beta = 30°$	−135	−87	0	+245	620	1100
	$\beta = 60°$	−70	+49	176	350	550	830
	$\beta = 90°$	0	0	0	0	0	0

15. Experimental Figures.

W. Margoulis has given* some experimental results for screws driven as windmills with their axes inclined to the wind. Some of these are given here in English (pound, feet, second) units in Table V. and Fig. 15. The pitch of the screw is 0.8D.

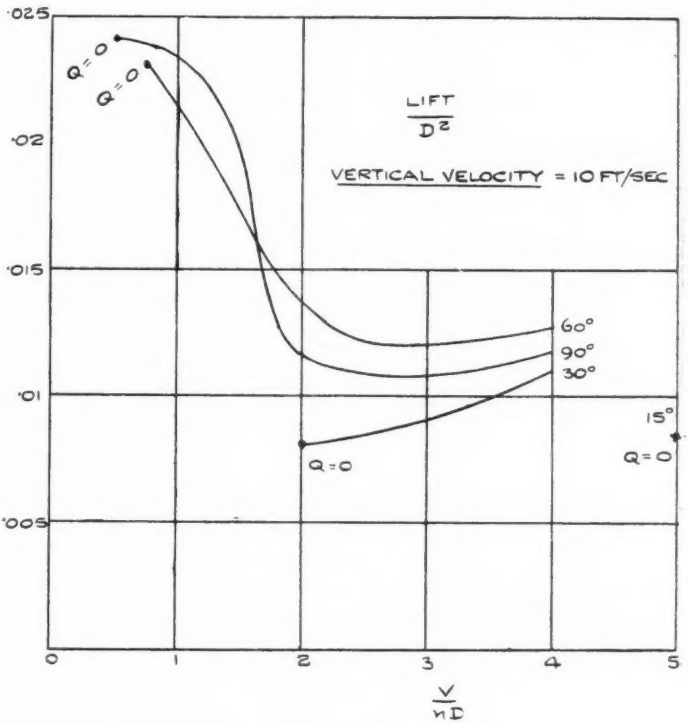


FIG. 15.

When the couple on the shaft is zero:—

β	15°	30°	60°	90°
V/nD	5	2	0.75	0.5
$10^6F_t/n^2D^4$ ...	—1470	—322	—130	—60
$10^6F_n/n^2D^4$ ...	755	0	0	0

From these figures have been deduced the curves of Fig. 15, showing the vertical lift when the machine is falling with the axis of the screw making different angles with the normal to the flight path. The curves are only plotted over a region where a braking couple (Q) is applied to the screw. The gliding angle is

* Supplement to L'Aéronautique, No. 34.

not found to be less than 60°. It will be seen that the lift is nowhere greater than when the airscrew axis is vertical and the torque zero. I do not think these figures are very conclusive; the pitch of the screw is too great and much better results should be obtained with a screw of pitch 0.2D or 0.3D.

16. Gyroscopic Forces.

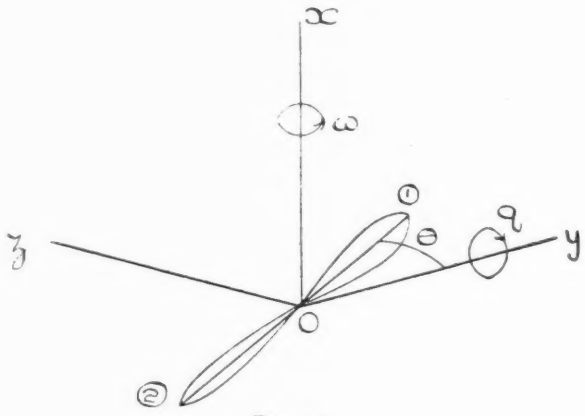


FIG. 16.

Let the axis of x be taken along the axis of rotation of the screw.

Let ω be the rotational speed of the screw.

θ be the angular position of blade (1) measured from the axis of y.

I_1 be the moment of inertia of the blade about Ox. Then, if $A, B, C, \ldots$ have their usual meanings, we have:—

$$A = I_1 \qquad D = I_1 \sin\theta\cos\theta$$
$$B = I_1 \sin^2\theta \qquad E = 0$$
$$C = I_1 \cos^2\theta \qquad F = 0$$

Now let the screw have an angular velocity q about Oy. Then the angular momenta are

$$h_1 = I_1\omega$$
$$h_2 = Bq = I_1 q \sin^2\theta$$
$$h_3 = -Dq = -I_1 q \sin\theta\cos\theta$$

The couples acting on blade (1) are:—

$$\text{About } Ox: \quad \dot{h}_1 + qh_3 = -\tfrac{1}{2}I_1 q^2 \sin 2\theta$$
$$,, \quad Oy: \quad \dot{h}_2 = I_1 q\omega \sin 2\theta$$
$$,, \quad Oz: \quad \dot{h}_3 - qh_1 = -I_1 q\omega\cos 2 - I_1 q\omega$$

If there are two blades the resultant couples are

$$-\tfrac{1}{2}Iq^2 \sin 2\theta \text{ about } Ox$$
$$I\omega q \sin 2\theta \text{ about } Oy$$
$$-I\omega q\,(1+\cos 2\theta) \text{ about } Oz$$

where I is the total moment of inertia.

If there are more than two blades the only couple is $-I\omega q$ about Oz.

These are the couples acting on the propeller; equal couples with opposite signs act on the helicopter.

With two similar propellers, each with two blades, rotating at the same speed in opposite directions, the couples acting on the helicopter are:—

$$-\tfrac{1}{2}Iq^2 (\sin 2\theta_1 - \sin 2\theta_2) \text{ about } Ox$$
$$I\omega q (\sin 2\theta_1 - \sin 2\theta_2) \text{ about } Oy$$
$$-I\omega q (\cos 2\theta_1 - \cos 2\theta_2) \text{ about } Oz$$

If q is small in comparison with ω the couple about Ox is negligible.

If the screws start rotation with their blades in line, or parallel if on different axes, all the couples acting on the machine will be zero. If they have more than two blades there will never be any resultant couple. In both cases, of course, the stresses involved by the couples balancing out among themselves will have to be considered.

(To be continued.)

STABILITY CALCULATIONS IN THE PROCESS OF DESIGN.

BY JOHN D. NORTH.

It cannot fail to be a subject for remark that in spite of the very large amount of work which has been done in this country on the subject of formal stability calculations, these calculations do not form a part of routine design of the majority of aircraft firms, nor are they used by the Air Ministry as a standard process of checking the aerodynamical qualities of an aeroplane. I use the term "formal" stability calculations to cover the solutions of the equations of motion of an aeroplane in rectilinear flight developed from Bryan's work.

From the remarks made by Commander Hunsaker in his Wilbur Wright Lecture it may be inferred that a similar situation exists in the United States, and I understand from M. Toussaint that such calculations are not customary in France. A discussion of the causes from which this situation has arisen will, I think, be of interest to those engaged in stability work.

The first and prime cause of this apparent neglect is a very simple one. The aircraft designer, whether he is employed by a commercial firm or by a Government department, is compelled to adjust his work to suit the specified requirements of his market. The aeroplane which is stable, in the true sense of the word, has a marketable value very little in excess of that of the machine which is reasonably easy to fly, and it has been found that by devoting the limited resources available to the improvement of performance better financial returns are assured. This may be, and I think is, an unsatisfactory state of affairs, but it nevertheless exists.

Secondly, the formal investigation of stability is not insisted upon by the Air Ministry in the manner in which the structural design is checked. A certain set routine has been adopted for the verification of the structural strength of aircraft, and is required as a condition of contract in the case of service machines, and by Act of Parliament so far as civil aeroplanes are concerned. There has, however, been no official pressure brought to bear to ensure that stability work which is not demanded by market conditions is carried out in the interests of service aviation or public safety.

Thirdly, it has never been completely demonstrated that the results of such calculations accurately represent the stability characteristics of the aeroplane as designed. Although many years ago an aeroplane, which was apparently stable over at least a part of its very limited speed range, was produced as the result of the worthy experimental efforts of the late Edward Busk and Robert Mayo; this aeroplane was not rendered stable by designing in accordance with the formal process, but by ingenious empiricism, trial and error, and devoted and dangerous experimental flying. It is true that this aeroplane has been the subject of subsequent calculations and flying tests, but these tests have not been sufficiently complete to verify the accuracy of the calculations, in fact in so far as longitudinal damping is concerned the values of M_q, the only rotary derivative deduced from quantitative tests, have not shown good agreement with the estimated figures.

When we consider all these facts, it is perhaps hardly surprising that those engaged in the design of aircraft do not embark on the long and arduous processes of developing a method of design which they are not satisfied will repay them for their work. If all performance testing had been confined to the casual reports of the pilot that the aeroplane appeared to fly fast or slow as the case may be, to have a good or moderate climb, or to reach a considerable or low altitude, there would hardly have been the incentive to improve performance

which exists as the result of the detailed investigations in this direction carried out at the official testing stations. Here, at least, there is a possibility of making a comparison of results with estimates without which it is difficult, if not impossible, to make any real progress in a design method.

Under present normal conditions it is only possible to judge the stability characteristics of an aeroplane from the qualitative report supplied by the pilot. A report, which even with the best intentions, is always somewhat unreliable and apt to be coloured by the characteristics of the machines which the pilot has been flying immediately prior to the tests in question. It has been my experience that so far as it is possible to judge the results of stability calculations from such reports, the formal process of stability calculation and design gives results which are even more satisfactory than might reasonably be expected from the known inaccuracies involved.

In order to verify, in some measure, the routine system of stability calculations which they had developed, Messrs. Boulton and Paul carried out a series of experiments with a small aeroplane which was definitely unstable over a large part of its speed range, by making modifications in this aeroplane and estimating their effect on the stability of the machine, and thus compared to some extent quantitatively their calculations with the full scale results obtained by observations of the air speeds at which certain phenomena occurred.

The results of these experiments were entirely satisfactory, although very limited in scope, and it was sufficient encouragement to justify further development of the routine process so that the calculations, which in the case of the first aeroplane extended over nearly a year, can now be carried out in a few weeks. It is hardly possible, however, to make any further real advance until accurate experimental results can be obtained; as the result of the calculations made in the course of design, the periodicity and damping of the long and short longitudinal oscillations over the whole of the flight range, in level, climbing and gliding flight, are determined, as are also the rolling and spiral subsidences and the yawing oscillation over the same range. Qualitatively, the machines seem to show results of flight which accord with expectation, but it is obvious that quantitative measurements are imperative to check the assumptions made.

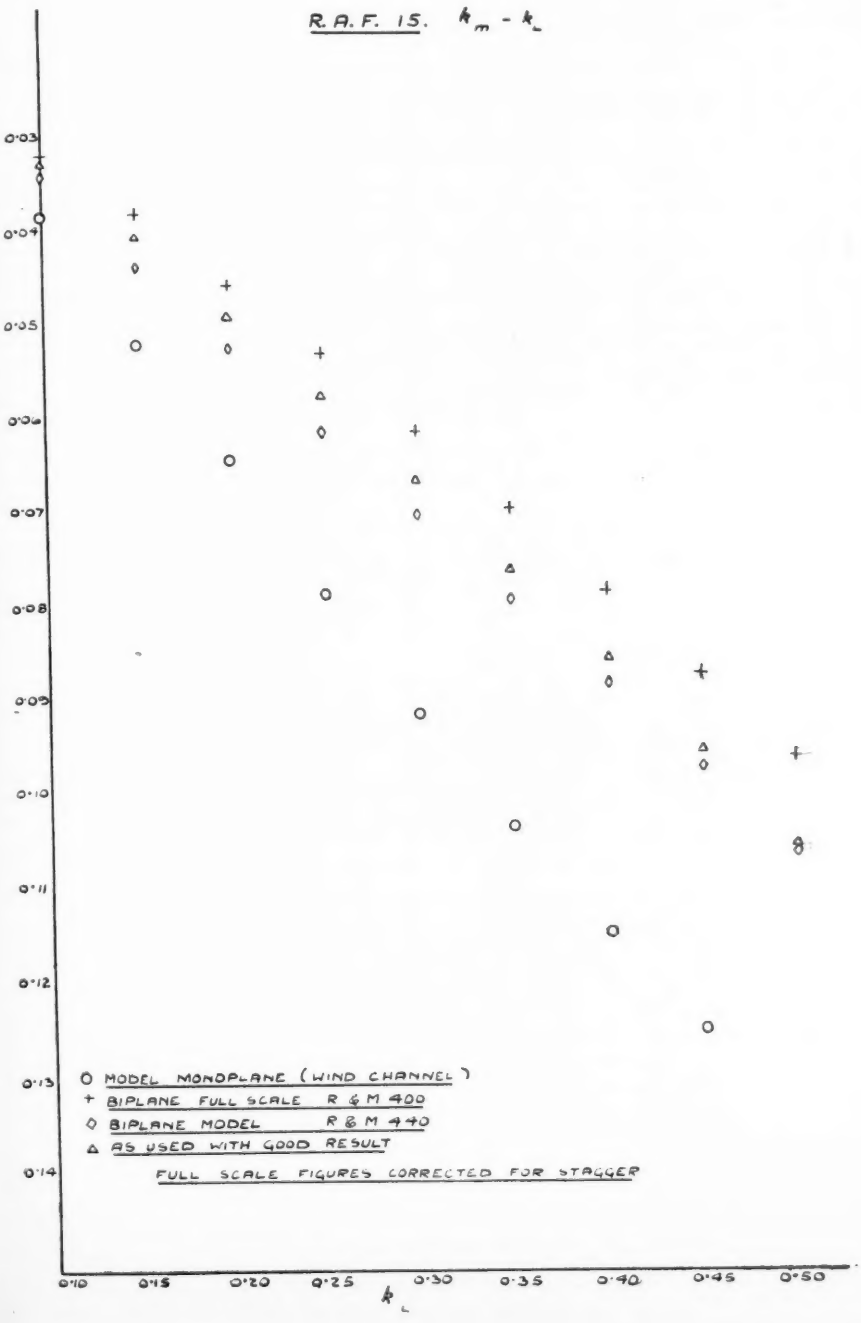
The principal difficulty experienced in the process of design is, of course, the estimation of the derivatives. With the exception of X_q it is possible to write down a series of expressions from which the derivatives may be calculated from existing aeronautical data coupled with certain specific wind channel tests, carried out on the ordinary balance. In addition to this, experiments may be made on an apparatus for directly determining the rotary derivatives, and experiments of this type have been carried out at the N.P.L. and elsewhere for some years past. Although such inference might not be drawn from the published results, these latter experimental measurements have only quite recently been satisfactory. Torsional oscillations in the spindle connecting the model with the indicating or recording apparatus had previously disturbed the experiments as to give rise to varying results with different moments of inertia of the apparatus.

Mr. A. R. Relf has recently pointed out the cause of these inaccuracies and has devised an apparatus whereby the motions of the model are transferred to the indicator or recorder by tension wires, and he has further introduced a system of forced oscillations previously only employed in the measurement of the compound derivatives L_r and N_p. Valuable evidence can be obtained by carrying out a series of tests with this apparatus and comparing the figures obtained with those given by the approximate expressions; there is, however, every reason to believe that wind channel results are not directly applicable to full scale flight, as scale effect is important in the value of the slope of the moment curve at various altitudes. Fig. 1 illustrates a number of model tests compared with those obtained by full scale experiment on the R.A.F. 15 wing section, and shows the large

discrepancies which may arise, even in the case of this section concerning which there is an exceptionally large amount of experimental data. In order to determine the relative attitude of the various surfaces under different conditions of flight, it is necessary to estimate very carefully the conditions governing the equilibrium of the machine, and these again are rendered doubtful by the uncertain value of the moments, while they are still further complicated by the presence of the slipstream, which affects not only the downwash but also the direction of the wind relative to the vertical surfaces. The variation or directional trim, engine on and engine off, is a problem of some importance in considering uncontrolled flight. To overcome this variation it has been proposed to set the engine obliquely in plan, but it is doubtful whether it is practicable to affect a complete trim on a high power machine in this manner. The value of the downwash angle from the main planes is also a matter of difficulty, as although this has been made the subject of considerable study the conditions from aeroplane to aeroplane are varied by interference effects. In considering lateral and directional stability, the effect of the airscrew on Y_v, N_v and N_r is very important, particularly in the case of N_v, in which it is responsible for the largest term. Approximate expressions have been suggested for estimating the side force on the airscrew due to yaw (see R. and M. 427), and by the use of these expressions it is possible to include the effect of the side force in the derivatives. The accuracy of these assumptions requires special checking and further investigations are required into the fin effect of the propeller stopped or running freely as in a glide. It is possible that experiments in the new N.P.L. duplex channel on a model complete with propeller may throw some light on this point.

For the longitudinal derivatives it is probably possible to obtain a fairly accurate forecast from existing data without special wind channel tests. This is due to the conventional forms of tail planes and sections in general use, and the relative unimportance of the body in M_w and M_q. If, however, there were any peculiarities in the design of the aeroplane, wind channel tests would certainly be necessary. In the case of lateral and directional stability the body is of more importance and the forms of the vertical surfaces vary greatly from machine to machine. Due to the desire to present a pleasing appearance to the eye, the fin and rudder are generally of curious outline aerodynamically, and an estimation of the characteristics of these surfaces and the interference effect of the body thereon would be a matter of extreme difficulty without wind channel tests. Under these circumstances the experimental determination of $dY/d\psi$ and $dN/d\psi$ for the body, fin and rudder are necessary. These tests are neither difficult nor expensive and they do not involve the delay and cost of manufacturing the complete model, the metal aerofoils of which represent an elaborate piece of work. It is not an exaggeration to say that a complete model of an aeroplane to be really useful for wind channel work, costs from £400 to £500 and takes several months to produce. The models necessary for the wind channel tests on the body, fin and rudder cost only a small fraction of this sum and can be produced in a few weeks. The characteristics of the aeroplane depending on the distribution of its mass can be ascertained without much difficulty and with a fair degree of accuracy. With the customary axes passing through the centre of gravity the Y axis is a principal axis by symmetry, the only product of inertia arising is in respect of the XZ axes, and has been found to be a term of negligible importance. The accuracy of the first calculations of the moment of inertia can be subsequently checked from the detail weights of the aeroplane and it would not be a difficult matter to verify these values experimentally, though the necessary rig would be somewhat expensive.

Stability calculations should properly form the basis of the estimation of the loads likely to come on an aeroplane flight and furnish the required basis for the determination of controllability. At the present time the loads on an aeroplane for purposes of strength calculations are assumed by a very rough and ready system, which has no real justification, and in view of the immense importance


R.A.F. 15. $k_m - k_L$
0·03
0·04
0·05
0·06
0·07
0·08
0·09
0·10
0·11
0·12
0·13
0·14
0·10
0·15
0·20
0·25
0·30
0·35
0·40
0·45
0·50
k_L
○ MODEL MONOPLANE (WIND CHANNEL)
+ BIPLANE FULL SCALE R & M 400
◇ BIPLANE MODEL R & M 440
△ AS USED WITH GOOD RESULT
FULL SCALE FIGURES CORRECTED FOR STAGGER

of weight economy, it is extremely likely that the material used in the construction of an aeroplane is not disposed of to its best advantage. So far as the estimation of controllability is concerned the difficulties of the ordinary stability calculations are accentuated, but even with the present information available it seems possible to get a very good idea of the control characteristics of an aeroplane with the assistance of wind channel data relating to the control surfaces. It is, of course, a fairly easy matter to appreciate after the calculations on a number of aeroplanes what are the important characteristics governing the stability of a machine, as for example the position of the centre of gravity, the sizes of the control and stabilising surfaces and the leverages through which they act. If it were not so these calculations, which are actually a process of trial and error, would be far more laborious than is actually the case, but in spite of the facility with which many of the characteristics of the stable aeroplane have been adopted by aircraft designers without the subsequent verification by stability calculations, accidents, apparently traceable to instability, still seem to arise and it is probable that they will continue to occur until stability work is seriously treated as a routine part of design and test flying.

GAS ARMOUR FOR AIRCRAFT.

BY F. L. M. BOOTHBY.

The greatest deterrent to the growth of air travel is the fear of fire in the minds of potential passengers. It is not a groundless fear, for there must be few members of this Society who have not lost friends and acquaintances from this cause. Unfortunately, the fuel at present in use in all aircraft is of a highly inflammable nature, as is the hydrogen with which airships are filled. So long, however, as oxygen can be kept from mixing with the gas or fuel a fire cannot occur. The problem of abolishing the risk of fire in the air resolves itself into finding the best method of excluding the oxygen from the combustible material. The means adopted must be light, otherwise the aircraft could not use them, cheap and capable of application in any part of the world.

In the exhaust gases from our internal combustion engines we have a completely non-flammable gas, which will render any fuel or gas safe so long as it is the only material in contact with it.

Every aircraft therefore carries its own antidote to the fire danger if only it can be applied. Let us consider the case of the aeroplane first. Here the danger of fire lies in the structure of the machine being set on fire by the engine or wireless apparatus; and secondly, fire in the fuel. Fire in the structure can generally be avoided by good design, and if it does occur is of a minor nature and can be dealt with by means of the usual fire extinguishing appliances. Fire in the fuel is a different matter. In peace time it may occur through a leak developing in the fuel tank or fuel system and so allowing an inflammable mixture to accumulate in the aeroplane. This mixture may be fired by a spark from a loose terminal in the wireless or other electrical gear, including the ignition system, by sparks from the exhaust, and also I am inclined to believe from the staticical discharge which may take place between the aeroplane and the ground, and certainly from the heavy sparking which occurs if the aerial is in use in a thundery state of the atmosphere. In war time we have to add the action of incendiary bullets to the means by which the fuel may be ignited.

Should the ordinary fuel tank spring a leak, accidentally or through hostile action, the escaping petrol gains immediate access to the air and the fuel and its vapour is free to go to any part of the aeroplane into which it may be blown. It is a simple matter, however, to enclose the petrol tank in an outer casing, which may be partly formed of the fabric fairing of the fuselage. From this outer casing a drain pipe may be carried to the bottom of the skid or other place of safety. It is also a simple matter to fill the space between the fuel tank and the outer casing with a supply of non-flammable gas by leading a small metal pipe into it from the exhaust pipe, this pipe being of sufficient length to cool the gas during its passage through it.

If now an incendiary bullet, or even a Very's light, is fired into the fuel it will not catch fire. This method of protection is quite satisfactory for small leaks, but trials of safety fuel tanks at Farnborough show that further steps must be taken to deal with the case of a large leak near the bottom of the tank. In this case it will be found that petrol may squirt from the hole under pressure and out of the hole, or holes, in the outer casing where, not being under the protection of the inert gas, it may be set fire to. The simplest method is to provide another layer of fabric or other material between the fuel tank and outer

casing so that the holes made would have to be in such a position that the petrol jet, falling under the action of gravity, would find the three holes in the correct position to allow it to reach the outer air. This might happen if a large number of rounds were fired close together, but is unlikely. It is better therefore to place a material such as a sponge, and preferably rubber sponge, outside the petrol container, as if this does not close the bullet hole and stop the leak entirely it reduces it to a slight trickle and the petrol, remaining in the inert gas, cannot be ignited. By passing a loose fabric sleeve doped with petrol proof dope, or lined with goldbeater's skin, over the petrol pipes it is possible to keep the whole fuel system under the protection of the gas armour up to and including the float chamber of the carburetter, and should a pipe break the petrol would flow back along this outer fabric pipe (it may be metal if desired) back to the casing of the petrol tank and from there drained away to a place of safety.

As an extra precaution it would be possible to place all accumulators, wireless gear, etc., in a gastight tank of fabric or other material and keep this fuel of inert gas, so that " shoots " occurring in a crash or otherwise and causing sparks would be unable to set fire to anything.

Airships.

In this type of aircraft the problem of protecting the fuel from fire is the same as in the aeroplane. In rigid airships it is the custom, at present, to stow the fuel along the keel and it should not be difficult to enclose the whole of the petrol tanks in a gastight fabric tunnel and fill the space round them with non-flammable gas. The main trouble is that exhaust gases are poisonous and arrangements would have to be made for blowing through the space round the tanks before they could be inspected.

In passenger airships the keel is required as a gangway for passengers entering or leaving the airship, in which case the petrol tanks are at present in the way and and would be more of a nuisance still if covered in and surrounded by a poisonous gas, while the increased weight is also an objection. It has long been the practice to carry the fuel tanks of non-rigid airships on the side of the ship and it is thought that this should also be done in rigids. The tanks should be built into the structure of the main horizontal longitudinals, when the metal used in the construction of the tanks would also add to the strength of the ship, which is not the case at present. If all the electric leads are carried along the bottom of the keel in a tube filled with non-flammable gas there would be little chance of the fuel being ignited should the ship break, as in R.38. Fortunately, we can now see our way to abolishing the use of petrol in airships, so that with the electric leads gas armoured and a heavy fuel stowed on the side of the ship there should be reasonable safety in peace practice, and gas armour round the tanks would protect them from incendiary attack in war.

We have now to consider the question of protecting the gas with which an airship is filled.

Experiments have proved that if you have a layer of non-flammable gas some six inches thick round hydrogen, it is possible to fire a Very's light into the hydrogen container. The Very's light will continue to burn till it has made a hole in the bottom of the hydrogen bag, and then in the outer cover through which it will fall, still incandescent, but without setting fire to the hydrogen. The same experiment may be conducted with incendiary bullets. The non-flammable gas must be at a slight pressure, so that air does not gain admittance through the holes made. It will be found that as soon as the non-flammable gas loses pressure air leaks in and hydrogen leaks out fairly rapidly, and a subsequent incendiary attack in the same locality will cause a fire. The problem then is how to maintain your protective gas under pressure. Supposing the space between the gasbags and outer cover to be completely filled with nitrogen, which is the

most suitable commercial gas for the purpose as it is no heavier than air, it can be maintained under pressure so long as the airship continues to rise slowly, and a very large hole would have to be made in her outer cover before the pressure would fall with the airship climbing at her maximum speed.

Helium would be a very desirable gas to use for protective purposes as its lift is within 5 per cent. of that of hydrogen. It is probable that in the fighting airship of the future double gasbags will be used, the outer filled with helium and the inner with hydrogen. It is highly improbable that hydrogen will be absent altogether in such an airship owing to its value as fuel, but on going into action the airship can rise till all the hydrogen is blown off and only helium remains.

For commercial purposes we must tackle the problem in rather a different way. We do not want the expense of buying helium or even nitrogen. We have available the non-flammable gas from the motors, but this has the disadvantage of being heavier than the air it displaces and the piping for cooling the gases down to a temperature which will permit of their being put round the hydrogen without damage to the fabric also involves weight. Nevertheless, it is considered to be worth it, an advantage gained is that the water in the exhaust will be condensed and fall to the bottom of the outer cover where it can be collected and used as ballast. A disadvantage is that it is poisonous and that the crew would have to use gas masks when working in its vicinity.

The extra weight involved will be these masks, say 100lbs., making outer cover proof to water and CO_2, and isolating the trunks through which hydrogen is valved to the atmosphere, say 500lbs. Gas valves in outer cover, which should be on top so that air will be pushed out of them as the exhaust gas enters at the bottom, say 100lbs., and the gas cooling apparatus on the engines at 100lbs. each, 600lbs., a total of 1,400lbs.

Distance pieces would have to be inserted between the gas containers and the outer cover to allow the protective gases to flow round which would slightly increase the above figures.

In a commercial ship we do not expect to have to deal with incendiary attack and the means by which our hydrogen may be set alight are reduced to fire in a car, electrical defect, or lightning. Ships have been lost in thunderstorms, but there is a case of a Zeppelin struck by lightning and having all her bow girders fused together in the nose without harm being done. If lightning could enter the gasbag without hydrogen coming in contact with air a fire could not occur. The most likely places for a ship to be struck are in the bow or stern, at the highest point amidships, where a lightning conductor should be placed and above the cars. The hot exhaust gases are good conductors of electricity and a ship under way in a thunderstorm leaves a conducting trail behind along which lightning may travel. If the aerial is out this is an additional source of danger. It is easy to place a small bag of non-flammable gas above each car, at the bow and stern and under the top lightning conductor. It only necessitates a ½in. pipe fed from the exhausts of the engines and led into these bags which can be open to the atmosphere through another small opening on the top, very little weight is involved, say 200lbs. all told. With similar protection over the passenger car it is probable that the gas in the commercial airship is as well protected as is necessary. These pads of non-flammable gas over the engine cars also give protection in the case of a fire in the car, provided it is a small one, as it should be, as there should not be sufficient fuel in a power car at any time to permit of a large fire. This gas protection over the engines was incorporated in the original design of the R.23 class, where the engines were carried in the keel, giving a saving of about two tons weight and an increased speed of two knots, compared to the modified design with cars. If the methods

suggested to protect the fuel from a defect in the electric wire leads are adopted they will also be sufficient in the case of the hydrogen.

Further experimental work is required to determine the thickness of the layer of non-flammable gas necessary to protect against incendiary attack in relation to the pressure at which it is maintained. The thicker the layer the greater will be the head resistance of the airship and the greater the sacrifice that will have to be made, exactly as in the case of armour protection in surface ships. Once the principle of armouring aircraft is adopted we may confidently look forward to the old contest of guns versus armour being fought out all over again in the air.